used almost as extensively on these as for its original purpose. It has been operated from Hampshire to the County of Durham, travelling over some of the most severe routes used by abnormal loads without serious hitch and always to a strict timetable, while conforming scrupulously to all regulations governing such movements.

137. The operating crew acquired an extraordinary skill in placing the bogies precisely on the positions required even when moving the machine in reverse, and in most unsuitable circumstances.

The Paper, which was received on 28 February, 1957, is accompanied by twenty-two sheets of diagrams and eight photographs from which the half-tone page plates and the Figures in the text have been prepared, and by two Appendices.

CORRESPONDENCE should be forwarded to reach the Institution before 15 August, 1957. Contributions should be limited to about 1,200 words.—SEC.

Discussion

The Chairman, in moving a vote of thanks to the Authors, said that the Paper dealt in a very interesting manner with the assessment of the strength of masonry arches by investigation of their movements and deflexions under load. It reminded him how an arch, by virtue of its shape, was never at rest. The weight of the crown and any live load on the arch tended to thrust the abutments outwards, although, as the Authors had shown, one could get quite unexpected movements from live loads at various places.

141. He hoped that the Authors would be able to add the dates of construction and a note on the shapes of the arches of the bridges that had been tested.

142. Had any significant differences been found in the results of the tests on arches of varying shapes? The three main shapes were the semi-circular, the pointed or ogival, and the elliptical or basket-handled arch. In Fig. 25, of the Yarm Bridge, the far arch appeared to be more or less of circular shape and the nearer arch was pointed. Had the Authors tested both, and, if so, had there been any significant differences?

143. Brunel's railway bridge at Maidenhead was perhaps one of the most famous of basket-handled arches. He wondered how it would respond to deflexion tests, and thought it would deflect considerably because of its high span/rise ratio. But it was a wonderful bridge and had given extraordinary service.

144. There was one further point he wished to ask. Who had first enunciated the rule of the middle third, and when?

Professor A. J. S. Pippard (Emeritus Professor of Civil Engineering and Senior Research Fellow, Imperial College of Science and Technology) confined his attention to three points only.

146. The first was the suggestion that elastic theory should be used for the design of the arch bridge. That had first been suggested by Castigliano, and his book contained a worked example of strain energy analysis applied to an actual voussoir arch bridge. Any parts found to be in tension were assumed to be cut out and a new analysis made on the resulting arch. If one carried that process sufficiently far, one would probably finish with an effective pin joint at the point where tension was first discovered. However that might be, the work done by himself, Miss Chitty, and other collaborators had led to the conclusion that the elastic approach was proper to the arch structure.

147. His second point was that of elastic behaviour, and the straight lines obtained by the Authors in their tests indicated that they had observed it in their arches. There was, however, rather more in it than they had indicated. If one measured the thrust produced

at the abutments of an elementary voussoir arch ring and plotted it against the values of a varying point load applied to a particular voussoir, it was found that four straight lines of different slopes were obtained. They corresponded at first to the complete elastic rib (encastré); and then, successively, to a rib with a pin at the load point, to one with a second pin on the other side of the arch, then to a statically determinate rib in which a third pin was formed at the skewback nearer to the load. The formation of the fourth "pin" at a still higher load produced a mechanism and the structure collapsed suddenly. Although the thrust had not been measured in the present experiments the Authors had plotted the crown displacement of the arches against the load applied, and in many instances two definite straight lines had resulted. That was particularly evident in Figs 1 to 4.

148. Was that not an indication that they had really got to the second regime in the structure? That was to say, they had started with encastré arches but when the load in the first one for example had reached about 50 tons, he thought they had effectively developed a pin at some point in the arch. The whole behaviour of the voussoir arch was very similar to that of a structure such as a portal in which plastic hinges developed successively until collapse occurred. In the arch there was, however, very little plasticity in the "hinges" and they could be taken as sample pin joints.

149. The middle-third theory had been the cause of great controversy for many years, and the theory of the arch had been bedevilled by it. The generally accepted enunciation was that if one could draw any linear arch for the applied load entirely within the middle-third of the arch ring, the structure would be stable in the sense used by Rankin, i.e., safe. Unfortunately any number of such lines could be drawn for an encastré arch and the problem was to determine which was the right one. Many suggestions were made for discriminating from the unlimited range of possible linear arches but the correct solution had not, he believed, been realized until he and his colleague had given it in 1937 or 1938. Each possible linear arch corresponded to definite and calculable movements of one abutment relative to the other. If one assumed the abutment to be fixed, there was one linear arch—and one only— which could be drawn, but if one allowed a specified relative movement of the abutments a different one was obtained, so that each of the infinity of possible linear arches corresponded a definite movement of one abutment relative to the other.

150. That was important in relation to a point mentioned in the Paper—that if an arch was assumed to be safe provided that one could draw any linear arch within the middle-third, one achieved much the same result, as if it were assumed that the linear arch for the structure with *fixed abutments* must lie within the middle-half.

Mr H. C. Adams (Assistant Chief Engineer, Ministry of Transport and Civil Aviation) said that the study of arches had always been fascinating to engineers, and always would be. One reason was that for most structures the stresses could be analysed mathematically; whereas in arch bridges, particularly masonry arch bridges, which included brick, there were so many other factors that it was not possible mathematically to do an accurate analysis. One result was that if a decision had to be taken on the strength of a masonry arch it was essential, in his judgement, for that decision to be taken by an engineer with a great deal of experience in the behaviour of arches after a close inspection of the arch to find out what had happened during its life. Most of those masonry arches were about 100 or more years old. They had been subjected to all sorts of traffic and weather; they had been soaked with water which had got through the road surface; often the spandrel walls had pushed out; sometimes there were cracks in the arch ring; often there had been movements in the abutments. Those factors might be, and often were, far more important than the ordinary computed stresses derived from the application of live load. Even when considering application of a live load, it should be remembered that the load was applied by wheels from a vehicle and was transferred to the arch ring through filling material which varied tremendously over the range of arches, and it was not certain how those forces came on to the ring. He agreed with the Authors that in the final analysis they must depend on the judgement of the engineer more than on the analysis of the mathematician.

152. In the case of masonry arches, different aspects had to be considered. First, there was the decision on the arch strength for ordinary loads, loads that were covered by vehicles within the range of "Construction and Use Regulations", broadly up to about 23 tons. Legal restrictions on bridges were generally confined to loads below that limit, and most arch bridges probably were above that limit in strength. Then there was the range—or rather two ranges—of abnormal load. Abnormal loads up to 150 tons could use any bridge in Britain not legally restricted, upon giving notice and indemnity; if the owner of the bridge thought that it was not strong enough, when he got the notice he could advise the haulier, but had no legal power to prevent the vehicle from going over. Therefore, if the strength of a bridge had been assessed above the limit of the "Construction and Use" range but below the 150-ton limit, those vehicles could not be prevented from going over; consequently the bridge might continue to deteriorate through overloading. Above the 150-ton limit there was control, because the load could only move by order of the Minister.

153. Those bridges should be considered from those different aspects. Within the first range, of comparatively low live load, any analysis might reasonably apply, but above that range, over which there was no control, there might be a gradual deterioration and it might be necessary to have re-assessments from time to time. Therefore, whatever one did in the way of assessment, one must continue to watch the bridge and, if necessary, to take action to strengthen it or restrict it.

154. In § 14 the Authors asked "What dispersion of load through the fill covering the arch ring can be assumed?" They suggested the 45° rule, which was commonly applied; Mr Adams agreed that within the low range when the arch was acting reasonably and elastically that rule might be applicable. Above that limit, however, there might be difficulty and more consideration should be given as to how the load came through the fill. In the higher range of abnormal loading, there were other factors which were difficult to assess.

155. In § 14 (2) the Authors had asked: "What allowance can be made for transverse strength of the bridge and the possibility of there being slab action?" Instead of slab action, if the arch ring was sound, there was likely to be arch action diagonally from the corners of the springing with increased moment of inertia towards the haunches, thus relieving the crown!

156. In § 14 (3) the Authors asked "To what extent can it be assumed that masonry arches behave elastically?" Within the range of lighter loads, and so long as masonry arches were regarded as taking compression only, he would agree with the Authors that the arch did behave elastically.

157. In § 14 (4) the question was "What is the effect of abutment movement?" That was a serious problem; almost invariably movement on the abutment was detrimental, but it depended on how it occurred. In the tests described an inward movement had taken place often as the vehicle was coming on to the bridge from live-load surcharge, which meant that there was an inward horizontal thrust in the fill. If the abutment moved, did it move from thrust on the abutment itself, did it move from thrust on the arch ring pulling on the abutment which would put much heavier stresses on the arch, or did it act by rotation of the abutment? All sorts of things could happen to the abutment. There was proof that in concrete arch bridges in tidal water where there was eccentric distribution of stress round the footing, when the buoyant effect of rising water was added with its uniform distribution, there could be a rotation of the abutment with effect on the arch ring, so that all sorts of different stresses occurred in the arch according to how the movement took place. Generally when there was evidence of movement of the abutment, it should be stabilized, possibly by grouting behind the abutment and grouting up the filling above the arch ring.

158. In regard to question (5) § 14, "What allowance can be made for the strength contribution of the fill, the parapet, and the spandrel walls?" he agreed that in the lower range all that should be neglected but in the higher ranges, where there were abnormal

loads coming on to the arch the larger distortions brought additional forces into play—action of the fill and the spandrel walls might help to tide one over an awkward period.

159. The final question was: "To what extent can mortar joints be assumed to carry tension?" He agreed that one should make no allowance for that, because those arches were always overloaded at some time in their life and breakdown occurred; therefore one could place no reliance on it.

160. One other valuable contribution of the Paper was the assurance that with skew arch bridges, up to a fairly good degree of skew, it could be assumed that they acted as square spans.

Dr Norman Davey (Head of Engineering Division, Building Research Station) observed that, in introducing the Paper, Mr Chettoe had referred to the work of the Building Research Station, not only on cast-iron bridges but on a selected number of masonry arches. His then colleague, Dr Redshaw—now Professor Redshaw—had made some laborious calculations on arch bridges based on the elastic theory. They were limited, however, by lack of knowledge on the actual construction of the bridges and the properties of the materials. In consequence, loading tests were resorted to. For that purpose a vehicle was selected with a long wheel-base and a single axle which could be loaded up to 27 tons. During the war, tests were made with bogie loads up to 40 tons. Since then that 40-ton bogie load had become inadequate, as the Authors had remarked.

162. Tests by the Building Research Station had indicated that arch-bridges of spans up to 45 ft which did not deflect more than 0·05 in. under a single axle load of 20 tons at the crown of the arch could in fact safely carry a bogie load of 40 tons at the same position. The deflexion, however, increased by 50 or 60%. He suggested that in Figs 1 to 8 the horizontal line marked "B.R.S. criterion" was incorrect, and that the criterion was really a point indicated by 0·08 in. deflexion for a 40-ton bogie load. In no case had the deflexions on the bridge tested by the Authors exceeded that figure for that loading. Using that same criterion, five out of twenty-two arches tested by the Building Research Station had failed to satisfy.

163. Could the Authors give the dead-load stresses for Crawley Down and Rudgwick Bridges? Also, in the case of Crawley Down Bridge at the springing intrados there was a calculated compression stress of 40 lb/sq. in. under maximum load. On Rudgwick Bridge at the same position there was 120 lb/sq. in. tension. Clearly there was a difference in the type of bridge and in its construction. Experience had shown that masonry arch bridges usually did fail in compression around the springing point. It might be the result of pinning in the way that Professor Pippard had described, and it was perhaps interesting, in connexion with what Professor Pippard had said, to refer to National Building Studies Research Paper No. 16, Fig 7 of which showed the load/deflexion curve to failure of an arch bridge loaded at the crown. First, up to 20 tons there was an elastic deflexion of 0·02 in., then there was elastic and plastic deformation of 0·2 in. up to 90 tons, followed by deflexion on yet another line up to 0·5 in. under a maximum load of 123 tons. Was that a reflexion of the effects that Professor Pippard had mentioned? Failure occurred at a point subsequently found on demolition to be the true springing of the arch. Unless the conditions behind an arch were known he did not see how an accurate estimate of its strength could be made.

164. What were the stresses already existing in the arch ring before the application of load, not only the dead-load stresses, but what he would like to call consolidation stresses? Masonry arches apparently were often in a state of precompression. He had attempted to measure the stresses existing at the crown intrados of an arch at Hanworth, very much like the Crawley Down arch, with a 45-ft span with a 27-in. ring, and had observed a compression stress there of about 100 lb/sq. in. Even with a 100-ton trailer at the crown of the arch there was still about 50 lb/sq. in. compression stress at the crown intrados. That was a result of the heavy precompression; he suggested that the Authors' figure of 85 lb/sq. in. tension, estimated at the crown intrados at Crawley Down under a 90-ton bogie, might not in fact have been there!

Mr J. S. Campbell (Assistant Civil Engineer (Works), British Transport Commission) said that he belonged to the fraternity who were looking for some easy method of assessing the capacity of arch bridges. At British Railways those who were interested in the matter had been looking for some sort of tool to be placed into their hands. They had had long experience in maintaining arch bridges because, as Mr Adams had mentioned, most of the arch bridges were more than 100 years old. Unfortunately, in no uncertain terms the Paper and speakers in the discussion had said there was no easy method.

166. British Railways were responsible for tens of thousands of arch bridges—thousands of under-bridges carrying railways and thousands of bridges over the railways carrying road traffic. They had for many years been a matter of concern, particularly when water got through the road surface or, in the case of rail bridges, through the track; then it definitely became a matter of assessment and judgement on the part of the engineer concerned. The Authors had, however, as a result of their test work, drawn attention to a range of factors about which there was more reasonable consistency and their use would form a basis for more realistic assessments.

167. He had been a little perturbed to hear Mr Adams say there were certain factors which could be neglected when assessing for light loads but that they should be taken into account in order to permit the travelling of heavier loads. That was the reverse of the way the railway engineer would want to treat a bridge, because he was responsible for the safety of the line underneath.

168. Surely a very important variable factor was the vehicle; he hoped the Authors might be able to arrange for a rigid standardization of the classification of vehicles using the road. His colleagues and himself on the railways would not want to have to give variations on account of the different kinds of vehicle, especially because of the important matter of distribution.

169. What did the Authors mean in § 31 in stating that when the load went on, the bridge "bounced" considerably? On reading that, Mr Campbell had felt like asking some of his colleagues to hurry along and have a look at the bridge—which was over a railway—because he felt sure that there must be something seriously wrong with it. Had they meant that there was a sudden impact or sudden blow rather than a bouncing? He was sure that an arch of that description could not set up resonance.

170. In § 34, it was stated: "Table 2 gives the effective width so derived from the test measurements together with the effective width derived from the assumption of 45° dispersal of load through the fill down to the level of the arch intrados," and it went on to say: "This simplification ignores any distributive effect which may take place in the brickwork itself as a result of slab action or, in other words, transverse distribution through the vault." How could one really get a deflexion and at the same time ignore the brickwork? The wording* might not be exactly right, but he had studied it for a while and had been unable to understand it.

Mr J. E. Jones (Divisional Road Engineer, Midland Division, Ministry of Transport and Civil Aviation) said that he could say a word to cheer up Mr Campbell. The bridge that had "bounced" was over a river and happened to be within the Ministry of Transport Division for which he was responsible. The bouncing had been brought to his notice in a casual manner by a member of his staff. When he went there himself and experienced it, he had as quickly as possible found enough money to fill with concrete! The bridge was of curious construction, and there were many reasons why there should have been vibration.

172. Part of the value of the Paper seemed to lie in the detailed and logical explanations of many phenomena which one had become accustomed to notice when inspecting bridges, and therefore he thought that, quite apart from the theoretical background, it had great value for people who had the care of such bridges.

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* The wording as printed in the advance copy of the Paper has now been amended by the Authors.—SEC.

173. He had always been a believer in filling such arches with concrete.

174. Mr Adams had already drawn attention to the possibility of an increase in the width of the working section of an arch vault towards the springing; in making elastic analyses of such arches, instead of taking a uniform foot-wide strip would it not be more logical to take the structure as a whole, or at least part of it, with the section increasing in the manner Mr Adams had described?

175. The Paper had returned a verdict of "not proven" on the problem of composite action. However, he wondered what the Authors meant by the term "composite action". Were they referring to the possibility of the arch filling acting as a composite arch with the main structural member, or was there not possibly some other type of action which perhaps ought not to be called composite action but which, nevertheless, was taking place? In Papers some years ago on the behaviour of cast-iron girder bridges, much value had been assigned to the composite action of girder bridges with filling about the girders. An arch bridge behaved in quite a different way, but the spandrel filling, provided that it was of suitable material, had through the years of constant traffic loading become consolidated and had formed a valuable structural member above the main one, even if that filling only restrained the movement of the structural vault; he thought it could do that to a greater extent than the mere dead weight of the material would allow.

Mr E. Longbottom (Head of Design Group in Ministry of Supply, Military Experimental Engineering Establishment, Christchurch) said it seemed, to say the least, doubtful that a criterion for the maximum permissible bogie load as determined by test should be a deflexion independent of the span and limited to 0·05 in. The Crawley Down Bridge would be rated on the full elastic analysis to about a 70-ton bogie load corresponding to a deflexion of 0·078 in. If that bridge had only been tested, the bogie load would have been limited to 45 tons. Would the Authors propose that any deflexion criterion be retained, or that it should be modified in the light of those test results?

177. Crown deflexions appeared to be measured relative to a fixed point beneath the arch and not relative to the abutments. Had the vertical sinking of abutments been so small that it did not materially affect the crown deflexions as measured? It was difficult to explain the relative deflexions at Rudgwick and Itchen Abbas without appealing to some such settlement. Both bridges were of 28-ft span, in cut, but on the face of it Itchen Abbas was a far better bridge. It had a better profile (3·3 against 4), a thicker ring (24 as against 18), a depth of filling of 18 as against 9, a greater effective width of about 14 ft as compared with the measured 13·2 of Rudgwick, and it had suffered materially smaller changes in span; and yet the crown deflexion at Itchen Abbas had been identical to that at Rudgwick up to loads of 60 tons and over that it had been 10% worse. From that equality of deflexion at Rudgwick and Itchen Abbas it appeared that the greater filling at Itchen Abbas had contributed nothing in the way of composite action. The satisfactory agreement between the calculated and observed crown deflexions at Crawley Down suggested that no account need be taken of composite action, but would it not be possible that the assumptions made and the constants adopted had rather fortuitously given agreement in deflexion and so masked possible composite action?

178. In §§ 48 and 49 the crown deflexions at Wetherby and Blythe End were compared with those at Rudgwick. Was the advantage at Wetherby the result largely of the effect of the concrete fill and had the potential advantage of the fill at Blythe End been offset by the suspected clay fill in that bridge? He agreed that, though composite action might occur, it was, as the Authors suggested, safer to ignore it in that any allowance for its action was relatively indeterminate; in certain cases the degree of action might be affected by weather conditions and its effectiveness might be progressively broken down by successive runs of heavy vehicles.

179. The present position on assessment of masonry arch bridges was that an elastic stress analysis supported by engineering judgement was the correct approach. Would the Authors agree that even if it were possible to test a bridge by the application of a load, that was no solution to the problem in that there was still no satisfactory criterion for deflexion and that, even with such a criterion, the successful application of that one load

was no guarantee of the ability of the bridge to carry further loads? It seemed, then, that analysis, supported inevitably by empirical rules to allow for abutment size and material, and so on, was inevitable. To that end, it would appear that further tests, accompanied by careful site investigations and supported by elastic analysis, were needed in order to get those empirical factors? He felt that any elaborate soil mechanics investigation was not worth while, for results could not be applied quantitatively in such an analysis.

*** **Mr B. G. Drummond** observed that it had been his task until 20 months previously to assist the Authors with several of the tests described in the Paper and with some of the calculations involved in producing theoretical stresses and deflexions.

181. The deflexion calculations were not without interest; it had at first been hoped to achieve a systematic method for arriving at theoretical deflexions for arches of varying section, thus allowing consideration to be given to increases in effective width for sections remote from the load (mentioned in § 45) as well as permitting the treatment of arches of non-uniform ring thickness. Unless the variation in section could be expressed mathematically in a form which was readily integrated when it appeared in the expressions for deflexion, the best approach appeared to be to perform the integrations by the summation method normally employed for arch stress calculations.

182. The scheme had been to evaluate deflexions at $\frac{1}{8}$ span points for a unit load placed at successive $\frac{1}{8}$ span points. Integration by summation had been used in conjunction with the slope-deflexion method but as soon as a few values had been calculated it had been found that points which should have equated by the reciprocal theorem were in such marked disagreement that something radically wrong was indicated. Attention had then been turned to strain energy methods but the discrepancies were still present in the deflexions.

183. The reasons for that rather discouraging inaccuracy had been found to be:—

(*a*) The expressions for deflexions involved a number of terms of more or less comparable value which were added and subtracted in order to produce the deflexion. The actual value of the deflexion might be of the order of 5% of those figures; it was a small difference between large numbers. Obviously accuracy in the large numbers was a prerequisite of accuracy in the small difference.

(*b*) An investigation of the exact assumptions involved in the summation method of integration had shown that, unless a vast number of elements were used, errors of at least 2 or 3% were unavoidable in the evaluation of individual integrations. The errors had been sufficiently large to put that method of computation quite out of count so far as deflexions were concerned. They had arisen almost entirely out of the assumption that the centroid of the moment acting on an element was coincident with the centroid of the element. That was true only when there was no moment variation across the element—a condition which rarely occurred with arches or any other structure. It would be possible to minimize the error by treating each element on its merits and allowing for the discrepancy on centroids, but that would involve prohibitive quantities of additional calculation.

184. It was interesting to note that the assumption of coincident centroids would give under-or-over-estimations for slope-deflexion methods and for strain-energy methods, except, in the latter case, for the deflexion actually under a load, which would always be under-estimated; i.e., the total strain energy was under-estimated.

185. It had then been decided, since most of the arches were of uniform ring thickness, to restrict the calculations to uniform section arches, where all the integrations could be done mathematically. That simplified and shortened the work considerably. It had been possible to systematize the calculations into a series of Tables. All multiplication

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*** This contribution was received in writing after the closure of the oral discussion.—Sec.

and division had been done using five-figure logarithms and no further disagreements with the reciprocal theorem had been encountered, other than those traceable to arithmetical error. The reciprocal theorem did in fact act as a very useful check on the arithmetic.

186. It had been desirable to split the calculations into sections so as to be able to show deflexions due to the load alone, due to rib-shortening alone, including deflexions due to flexure and compression, and due to the various things that could happen to abutments—spread, rotation, and differential settlement. From that spliting-up it was possible to see, amongst other things, that whilst the deflexion due to direct axial compression was generally quite negligible the deflexion following flexure due to rib-shortening might contribute as much as 30% to the total deflexion. In their final form the deflexions had been expressed in terms of the ring thickness and radius of the arch and related of course to a single shape of arch ring.

187. The Authors had given details of the measuring instruments used and the test vehicle. The deflectometer was a truly admirable piece of equipment whose reliability was perhaps matched only by the unwavering helpfulness of the crew responsible for the test vehicle under generally difficult and uncomfortable conditions. The spread gauge was engaged on the measurement of a much less consistent movement and it had sometimes been a moot point whether it was doing its job correctly. However, with experience, and by keeping a tally on the movements of the vehicle overhead it had often been possible to find a pattern in its variations which could be related to the positions of the vehicle.

188. The Authors had collated much information resulting from their tests and had provided explanations for many of the phenomena observed. They would perhaps be the first to admit that any masonry arch was so full of imponderables, so full of things that might or might not add to its structural strength, that there were a number of quite different ways of explaining any particular observation. That was not intended in any way to detract from the Authors' remarks but rather to reinforce the conclusion they had arrived at with regard to assessment.

189. The assessment of a bridge's strength had to be realistic but it did not need to produce a value for the bridge's ultimate strength. An adequate assessment that would show without reasonable doubt that the bridge would withstand the loads demanded of it was all that was required.

190. In putting forward their suggested method the Authors had chosen to neglect all the nebulous quantities involved in a masonry arch and were proposing that assessment be based on elastic analysis of the vault treated as a fixed arch operated on by the dead load. In that they were selecting the only available structural component whose action could be assumed beyond reasonable doubt. Since the components, such as spandrels, fill, etc., were not considered, any results which were obtained would tend to err on the side of underassessment, but that was not significant provided that the assessment was adequate. By and large, the Authors' figures indicated that assessment based on that assumption would be satisfactory for many present-day loads. Whilst the method of assessment might not satisfy the purists it did almost certainly present the best way now known of getting reasonable and comparative figures by any method more critical than visual inspection and judgement.

191. Should their proposals be adopted the Authors suggested the production of standard charts for assessment. The great majority of arches were of single span, uniform ring thickness, and either segmental (of rise/span ratio varying between 1/3 and 1/5) or elliptic and therefore tended themselves to standardization of analysis. Some work had been done on that subject and indicated that from standard charts and tables it would be quite possible to say whether the passage of a certain vehicle over a particular bridge of normal characteristics was a reasonable proposition after not more than 15 min of simple arithmetical calculation. That would include evaluation of dead- and live-load stresses and allowances for rib-shortening, spread, etc., for the critical load position, which could normally be selected by judgement where 45° dispersion was allowed.

Mr Chettoe, in reply, said that he would welcome a test of Maidenhead Bridge (§ 143) if it could be done. It was a magnificent bridge, although when just built a number of people had, he believed, said that it would not stand up. The bridges tested had all been built in the nineteenth century, except that at Yarm. The Yarm bridge, as it now stood, had been originally erected in about 1400 but had had major repairs[5] and alterations at various times between 1562 and 1788.

193. The "middle-third" rule appeared first to have been enunciated by Navier in 1826.

194. Professor Pippard's early work had been of great help in giving a clear idea of how voussoir arches behaved. Mr Chettoe thought that pins would develop when the thrust line approached the extrados or intrados and crushing was beginning. That point did not appear to have been reached in the tests and the slight changes in direction visible in the deflexion curves seemed more likely to be due to the first gradual step towards the formation of a pin—that was, the opening of a tension joint.

195. It was not possible, of course, to make an accurate mathematical analysis, as Mr Adams had observed. The Authors had suggested merely that arches in good condition did behave elastically and that was a reasonable basis on which to start an investigation. Later, the engineer would have to modify whatever results he obtained from elastic analysis to cover any defects (or the converse) which he might find. For example, if the abutments moved or if the arch cracked he would have to make due allowances and adjust the results of analysis according to what he saw. There might, perhaps, be bridges in such a state that it was not worth while starting an analysis and where the only comment to be made was a recommendation for immediate replacement. If that were not possible, the analysis would have to be made by guess-work and judgement, including knowledge, if possible, of how the arch had behaved under load previously. But Mr Chettoe thought that that was exceptional and that in the normal way the method could be used as a basis.

196. The Authors were trying, in particular, to suggest a method of assessment for loads that could be applied indefinitely without damaging the bridge; an assessment derived as proposed might be within the normal (Construction and Use) range or, usually, well beyond it. If, as might sometimes be the case, it was necessary (perhaps temporarily) to go higher, then use could be made of imponderable factors, including for example the greater dispersion through deeper fill away from the crown, if it existed, and, as Mr Adams had said, it would be necessary to examine the arch and review the assessment from time to time. The help that filling could give in those later stages would probably vary, one factor being the presence or otherwise of loads elsewhere than at the crown. Filling would certainly act in restraining the uplift of side spans when a middle span was loaded.

197. Mr Chettoe agreed with Mr Adams's comments on the triangular effect of the slab, but it sometimes happened that a fairly wide vehicle crossed a narrow bridge. If the fill was thick, the dispersal area transversely was considerable and might extend to the edge of the arch at the crown; in that case, full advantage could not be taken of the transverse dispersal near the springings. With a wide bridge it would be reasonable to do so, as Mr Adams had suggested, but the Authors had preferred to consider it as an additional safety factor—which was simpler.

198. Dr Davey had referred to the tests carried out at the Building Research Station. They had indeed comprised the main bulk of the work done in the investigations.

199. It was not surprising that dead load was sometimes found to produce compression at the crown; Mr Chettoe thought that was often likely to be the case because of the wedge action of the loads passing across the abutments and the filling behind them. As failure approached, pins might well develop at the springings and elsewhere.

200. He had, himself, a very strong hankering after composite action (§ 175) and had from time to time been restrained in that respect by Mr Henderson. It seemed clear that the results which had been obtained in the tests did not justify the assumption of composite action. It obviously occurred to some extent but it was modified by the possible breakdown which seemed to have happened—at least, at Yarm. In any case, most

[5] E. Jervoise, "The ancient bridges of the North of England". Arch. Press, London, 1931, p. 57.

contributors to the discussion seemed to agree on the need for caution. The Authors had not stated that allowance should *never* be made; they had suggested means by which some allowance *might* be made.

201. Mr Chettoe agreed with Mr Jones about the value of filling over the arch with concrete. It should not be forgotten, however, that such filling made the arch stiffer; and if any suspicion attached to the abutments they also should be strengthened.

202. The Authors had taken note of Mr Campbell's comment on the wording of § 34 as printed in the advance copy and had accordingly amended it.

Mr Henderson, in reply, said that the various points raised in discussion had emphasized the number of imponderables that were associated with masonry arch bridges. No two of them were the same, and he felt confirmed in his opinion that any method of calculation must be treated essentially as a guide to assessment. Mr Adams had put the matter succinctly when he stressed that the final decision must be one of judgement, taken by an engineer who knew his subject after a thorough inspection of the arch and the site.

204. He considered that, as pointed out by Mr Adams, the way in which the load came through the fill, particularly when considering the heavier classes of load, was a matter for careful consideration. The suggested dispersal at 45° was a general rule which might require modification in special circumstances. The other factors involved were, however, so difficult to assess, and so much more insusceptible to formulation and calculation that allowance for them seemed best left to engineering judgement. Mr Longbottom had referred to soil mechanics investigations; Mr Henderson agreed that the results of those would be very difficult to apply quantitatively in an analysis of a structure, and he baulked at the problem confronting them if an arch assessment involved an analysis which took into consideration quantitatively varying vertical and surcharge loads, following a soil mechanics study at each site. A general study of the characteristic behaviour of different types of fill would be useful, and indeed was an essential prerequisite in any attempt to assess the strength of a masonry bridge, but allowance for the properties of the fill was bound to remain largely a matter of judgement.

205. In the Paper, attention had been drawn to the greater width of arch which was effective, the greater the distance from the point of application of the load, and it had been suggested that that might be taken into consideration in calculation. The difficulties of doing so were twofold. First, whilst they recognized that the condition existed, the Authors felt that the formulation of a sound rule to express it was well nigh impossible, since it would necessarily require to be a function both of the width of the bridge and the distance of the point considered from the load. Secondly, if such a rule were possible, it would greatly complicate any analysis, since, apart from other considerations, the arch would have to be analysed twice—for dead load as having a constant moment of inertia, and for live load with a variable moment of inertia. It seemed doubtful if such refinements were worth while, even if they were possible.

206. It was undoubtedly true, as indicated by Mr Adams, that loads could use a bridge and cause a gradual deterioration of the structure, so that it was necessary to keep it under observation and perhaps amend the assessment. At what point that deterioration was initiated was a matter which the Authors hoped might be more precisely settled by the analysis of bridges now carrying heavy loads. It was something which could not readily be found by testing since it was a comparatively slow process.

207. Both Mr Adams and Mr Jones had referred to the strengthening of masonry arch bridges by grouting the fill, or by infilling of the spandrels with lean concrete. Grouting behind abutments in order to inhibit movement there was a very useful expedient where abutment movement was suspected to be excessive. So far as grouting the spandrel fill or removing it and replacing it with weak concrete was concerned, both Authors had, like Mr Adams and Mr Jones, long been advocates of it. The method was an excellent way of strengthening masonry arch bridges; given a reasonable thickness over the crown of the masonry arch it was probably safe to say that in almost every case it would be possible to make the structure sound for most abnormal loads if one or other of those

methods was adopted. It should not be overlooked, however, that that made the arch stiffer, so that a sound resistance to thrust ought also to be provided. It was also generally desirable to reinforce over the crown on the lower side of concrete infill.

208. Professor Pippard had pointed out that in his model experiments a curve of thrust plotted against load was made up of a series of straight lines (joined by curves), increasing in slope as virtual pins were formed progressively. It was of course, impossible to measure thrust in those structures. Had it been possible it would have given a much easier and more satisfactory basis for the experimental study. As Professor Pippard had suggested, it appeared true that the slope of the deflexion curve would increase as did the thrust curve, and at the same points. The deflexion curves in Figs 1, 2, 3, and 4 certainly did show such a change in slope and that could indicate the early stages of initiation of a virtual hinge, or, as the Authors would prefer to put it, the opening of a crack and the change from the arch working on its full section to a reduced section.

209. They had commented on the significance of that in discussing the deflexions at Rudgwick Bridge (§ 56). Dr Davey had also referred to the phenomenon in describing the effects of load on one of the arches he had tested. It seemed clear, however, that that first change of slope was a long way from the collapse load, provided there were no other serious defects in the structure. The Authors thought, however, that it was probably somewhere not far above that region where one could say: "loads above this amount will cause deterioration". Such deterioration would not, of course, manifest itself at once, but would only become evident after repeated applications of load.

210. It was also worth noting that that point of change of slope would vary from bridge to bridge, even if the bridges were otherwise identical, if the tensile strength of the mortar varied. Thus, if there were no tensile strength at all, the point would be lower than if the mortar could sustain some tension.

211. The Authors hoped, however, that by setting a limit to eccentricity where they had suggested, they had avoided serious encroachment beyond the load at which the slope changed. It might, however, be worth reiterating that those limits were associated in their minds with bridges in good condition and with sound backing. If Fig. 3 (Sutton Scotney Bridge) were examined again, it would be noted that the curve of deflexion against load curved upwards throughout the test. It had, unfortunately, not been possible to measure abutment movement in that test, but, situated as that bridge was between approach embankments which did not appear to give very adequate backing and with thin walls, it seemed almost certain that there was considerable abutment movement there, which would account for the progressive increase in the slope of the deflexion curve. Mr Henderson drew attention to the fact that the nature of the abutments and their backing would, by inspection and judgement lead to a reduction in the assessment of the bridge, particularly since it was of short span and considerable thickness, and so the more susceptible to the harmful effects of abutment movement. That structure seemed to him an excellent example of the way the test results confirmed a conclusion which would have been reached by an experienced engineer.

212. Dr Davey, he thought, had raised several valuable points. Mr Henderson wished in the first place to correct what was rather a loose use on the Authors' part of the phrase "B.R.S. criterion" for "0·05 deflexion". He agreed with Dr Davey that in Research Paper No. 16, the basis suggested was that, if the deflexion did not exceed 0·05 in. under a 20-ton axle load, the bridge was satisfactory for a bogie load of 40 tons. Since the test vehicle they had used applied twin and not single axle loads, the B.R.S. had, however, suggested that a reasonable deflexion to be taken as a general criterion for load assessments as a continuous function of deflexion would be that a bridge could safely carry that bogie load for which it just satisfied the same limitations as given for a single 20-ton axle—that was to say, 0·05 in. deflexion (see footnote to § 7). Unfortunately the Authors had slipped into the habit, in their discussions, of referring to it as "The B.R.S. criterion" and the phrase had become familiar to them. The correction was now shown against Figs 1 to 8.

213. Some approximate assessments of bridges tested had been made, using as a basis the test vehicle, and assuming in each case that the abutments were rigid and that the

arch was working on its full span. On the basis of the method suggested, calculated basic assessments for those bridges would be approximately as shown in Table 5. If those assessments were considered in relation to the corresponding deflexions taken from the appropriate figures, it would be seen that they seemed to lie fairly closely to the 0·05 in. value, provided the following points were kept in mind.

TABLE 5

Bridge	Approx. bogie load: tons	Corresponding deflexion: inch
Sutton Scotney	80	0·133
Rudgwick	40	0·044
Wetherby	40–45	0·035
Blythe End	50	0·058
Crawley Down	70	0·078

(i) Sutton Scotney Bridge, as had been previously pointed out, lay between very inadequately backed abutments. They would properly have led to a marked reduction in assessment on the grounds of engineering judgement.
(ii) Wetherby Bridge was very well filled with a lime concrete, substantially backed, and well haunched. It would not have been out of place in that case to have used a rather shorter span, or to have taken into account, by judgement, the exceptional fill at the site.
(iii) Crawley Down Bridge had a span considerably greater than Mr Henderson understood was considered in the B.R.S. recommendations.

214. In arriving at those assessments the Authors had assumed that dispersion took place down to the point where the arch axis was cut by the 45° plane. It would be realized that the calculated assessment was bound to be very sensitive to the size of the dispersal area assumed and also to the density taken for the dead load. That was largely the result of the low stresses and small eccentricities involved, and in those circumstances very careful judgement was necessary in each case as to what values should be taken.

215. Mr Adams had drawn attention to another aspect of live loading; the horizontal thrust due to surcharge from bogie loads in the vicinity of the abutments. That thrust was bound to be present to a greater or less degree, depending on how well consolidated the fill was. How it could be estimated quantitatively the Authors found it impossible to say, and they felt it best that it should be taken as a factor "kept in mind" when reaching final conclusions.

216. Dr Davey had drawn attention to the extent to which "consolidation" as well as dead-load stresses could exist in a vault and had referred to some experiments in which he had measured them. In computing stresses, it was, of course, impossible to estimate the amount of stress due to distortion of the arch and compaction of the fill. It seemed from what was said that the specimen tested was taken from the crown intrados after removal of fill, probably down to the haunching. If that was so would it not be the case that the arch intrados was under some degree of compressive stress due to a hogging moment at the crown induced by dead load in the haunches, together with the thrust due to self-weight? In addition, of course, any reduction of span due to surcharge effects would increase the hogging moment and so the compressive stress at the crown intrados.

217. Dr Davey had asked what were the dead-load stresses for Crawley Down and Rudgwick Bridges at the springing. By calculation they appeared to be approximately:—

	Extrados	Intrados
Crawley Down	10 lb/sq. in. tension	180 lb/sq. in. compression
Rudgwick	5 „ „	80 „ „

Live load at the crown of course caused a moment at the springing which tended to put the intrados into tension and the extrados into compression.

218. Dr Davey had pointed out that such bridges generally failed in compression at a point subsequently found to be the springing; in the case to which he had referred (B.R.S. Research Paper No. 16, Fig 7) that point of failure was between $\frac{3}{4}$ and $\frac{5}{8}$ span, compression failure being on the intrados. Whilst Mr Henderson agreed that failure would normally take place there, he felt that that point ought not to be defined as the effective springing of the arch when considering elastic behaviour.

219. The Authors had built a 3-ft-wide brick arch, 20 ft in span, and had tested it to collapse. It had neither haunching nor fill, but at the point of collapse (when the load began to fall away) it had exhibited identical phenomena—conspicuous hinges at the crown extrados and the intrados in the vicinity of the $\frac{1}{4}$ point (see Fig 31). It did not seem satisfactory to refer to those points at the quarters as springings, for the arch clearly extended from skewback to skewback. They were rather the additional pins which had to be formed to bring about a collapse mechanism.

220. That was not to suggest that it was necessary to assume the span as the distance between skewbacks. He thought it would be quite legitimate to consider the span as somewhat reduced if the ends were soundly haunched, treating the part of the arch near the skewback as part of the abutment, but it seemed essential before that was done to be sure that the haunching was really stout and thoroughly keyed to the arch so that the springing effects were well dispersed into it, and caused negligible deformation under load.

221. In reply to Mr Longbottom, Mr Henderson said that an attempt had at one stage been made to measure vertical abutment displacements, but it had presented great difficulty, since it involved providing a datum point remote from the bridge. From the general pattern of deflexions round the vaults, he considered, however, that such movements in the arches tested were small enough to be negligible. Mr Longbottom had instanced, in that respect, that the thicker vault at Itchen Abbas had crown deflexions very similar to those at Rudgwick. Itchen Abbas Bridge was, however, elliptical, whilst Rudgwick was segmental, so that, over the central parts of the arch the former was, in fact, a much flatter structure. Moreover, differences in the value of E could affect the deflexion materially. As Mr Longbottom had pointed out, it might well be that composite action took place, stiffening the various arches, and that at Crawley Down, for instance, the value of E was somewhat different from what had been assumed. The Authors' point was that they could not find in the test results evidence which could be attributed solely to composite action and not to differences in the material of the structure, although they were sure that composite action took place in many cases. It seemed quite reasonable for an engineer to augment an assessment in conditions where he was sure that composite action was taking place effectively, forming, as Mr Jones had described, a structural member above the main one, but, so far as the Authors could see, that modification would have to depend entirely on the engineer's judgement.

222. The Authors regretted not being able to give the easy method of assessment which Mr Campbell had hoped for. Masonry arches were, however, very complicated structures, and the more they worked with them, the more hesitant they became with over-simplified rule-of-thumb approaches. The method they now suggested was still a simplification of the problem; but they considered that, if adopted, it could be systematized into tables or charts which would greatly reduce the work involved. A certain amount of codification was perhaps desirable, and it might be that, in the light of experience from the proved capacity of many bridges, the limits of stress and eccentricity could be modified, but it did seem unavoidable that some approach based on elastic theory and taking into account as many of the variables as possible had to be adopted if a general basis of masonry-arch assessment was desired which would avoid testing of individual structures.

Fig. 31